

Financial Literacy Externalities*

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Abstract

This paper uses unique administrative data and a quasi-field experiment of exogenous refugee allocation in Sweden to estimate effects of exposure to financially literate neighbors. It contributes evidence of causal impact of financial literacy and points to a social multiplier of financial education. Exposure promotes saving for retirement in the medium run and stockholding in the longer run, especially when neighbors have economics or business education, but only for educated or male-headed households. Findings point to knowledge transfer rather than mere imitation. We do not find significant effects on income or employment prospects, except for employment in the financial sector.

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1 Introduction

Confronted with the demographic transition and rapid financial innovation, households make complicated financial choices with important and lasting consequences on their economic well being. Research on financial literacy, developed over the past decade, has established widespread presence of financial illiteracy, as well as a strong correlation between low financial literacy and negative financial outcomes at the household level¹ Establishing causality from financial literacy to economic outcomes has been more challenging but of profound importance for policy choices in the presence of competing approaches to empowering households.² In principle, unobserved factors can lead an individual both to become financially literate and to have good financial outcomes, without a direct causal link between the two. Reverse causality is also plausible, e.g., from saving for retirement to learning about financial concepts. Moreover, even if there is an exogenous influence of financial literacy on outcomes, the cost effectiveness of suitable financial education programs may be challenged if they can only reach limited segments of the population (e.g., school children) and their long-lasting impact on behavior is not firmly established.³

Existing literature has focused on the role that own financial literacy can play for household outcomes and has sought to overcome the econometric problems through instruments for own financial literacy, either going back to early life events or looking at environmental factors.⁴ Although this instrumental variable approach has been useful, it is unavoidably open to the criticism that early life or environmental conditions can shape financial behavior through numerous unobserved channels beyond influencing the instrumented variable,

¹See Lusardi and Mitchell (2014) for an excellent survey. Outcomes include lack of saving for retirement, lower wealth, stock market non-participation, use of higher cost credit, being in credit arrears, and recently also wealth inequality (Lusardi and Mitchell, 2007; van Rooij et al., 2011; Disney and Gathergood, 2013; Lusardi and Mitchell, 2014; Lusardi et al., 2016).

²These include financial regulation, financial advice, and default options in addition to financial education.

³Hospido et al. (2016) recently found that financial education programs are effective in improving financial literacy test scores of treated school children. Alan and Ertac (2016) conduct experiments with an educational program in primary schools and find an educational program in primary schools effective for encouraging school children to exhibit greater patience when making intertemporal choices in incentivized experimental tasks, also three years later. Brown et al. (2015) exploit variation in the enactment of financial and economics education reforms in high school curricula within and across US states to show that reforms have significant (though moderate and opposite) effects on the debt-related outcomes of 19- to 29-year-olds: the tendency to hold debt and to run into repayment difficulties are somewhat reduced by financial education and increased by economics education.

⁴Instruments have included understanding of financial matters by parents as perceived by the respondent, self-reported mathematics grades at age 10, institutional changes affecting early education, or introduction of financial education requirements interacted with State spending on education. See, for example, Lusardi and Mitchell (2009), van Rooij et al. (2011), and Jappelli and Padula (2013). Political beliefs in the respondent's environment have been used on the assumption that more right wing beliefs are associated with greater financial knowledge (Bucher-Koenen and Lusardi, 2011).

own financial literacy.

This paper takes a different approach and is the first to study financial literacy externalities, defined as the potential for financially literate neighbors to have an exogenous (positive) influence on economic choices of households. Financial literacy externalities reinforce and extend the notion of human capital externalities, in the spirit of Acemoglu (1996) and Acemoglu and Angrist (2001), as motivations for suitable educational programs, in this case with financial content. Establishing the presence of such externalities for behavior over a longer horizon can strengthen significantly the case for promoting financial literacy, not only by showing a lasting exogenous effect on outcomes but also by demonstrating greater cost effectiveness of relevant programs resulting from a social multiplier.

In the context of establishing financial literacy externalities, a thorny identification issue is posed by the typically endogenous choice of neighborhood. Sorting into neighborhoods with greater financial literacy may arise from unobserved characteristics of people (such as interest in financial matters) and of the area (such as availability of financial services and advice) and thus correlate with good financial choices without implying causality. We are able to tackle this issue by utilizing high-quality administrative data and tracking over a twenty-year period a group of people initially allocated to apartments by a government agency: refugees assigned to specific apartments through a nation-wide placement program.

Exploiting variation in financial literacy at the neighborhood of exogenous initial placement, we study financial behavior ten to twenty years later to uncover lasting exogenous effects of financial literacy in the neighborhood of initial placement over the medium and longer runs. As we know the precise location of refugee immigrants, we can also control for unobserved features of the greater area (parish) to which the initial neighborhood (electoral district) belongs.⁵ We explore channels through which financial literature externalities operate, including content, ability to process information, likelihood of interaction, salience, imitation, and labor market channels. The use of a refugee sample serves as a useful identification device of long lasting effects on economic behavior without use of instruments, but is also interesting in its own right. In view of considerable current debate on accepting and placing refugees, our analysis is additionally able to uncover long-lasting effects of the initial placement of refugees on their subsequent economic behavior.

We find more sizeable effects of the share of neighbors with specialized knowledge of economics or business rather than of those with quantitative education in general. Medium-run effects are observed on participation in retirement accounts, while longer-run effects

⁵Relevant features of this broader shared environment include the quality of public amenities and the penetration of the financial sector in a given neighborhood (Oreopoulos, 2003; Manski, 1993). As noted by Manski (1993), these ‘correlated’ effects are not social effects, and are not created by social interactions (Damm and Dustmann, 2014).

are seen on stocks, the more information-intensive and initially less salient asset. Importantly, externalities are operative only for the more educated refugees and for male-headed households. The share of neighbors who actively contribute to retirement saving on private accounts has significant, though smaller, effects on refugee financial behavior than having financially literate neighbors, suggesting that imitation is less important than transfer of relevant knowledge in this context.⁶ We find no influence of the initial share of financially literate neighbors on future employment and locational prospects of the refugees, except in encouraging them to get a job in the financial sector. All in all, our findings suggest that financial literacy externalities involve the transfer, processing, and salience of information, as well as confidence to apply it. On the immigration front, the paper shows that the initial environment in which refugees are placed can have lasting effects on their economic behavior.

In addition to financial literacy, our paper links to two other strands of literature. One studies peer effects on financial behavior, following seminal work by Duflo and Saez (2002), who found evidence that observing a higher share of workplace peers invest in a particular retirement product increases the probability that the respondent will also invest in the product.⁷ The other strand studies immigrant financial behavior with an emphasis on establishing links to culture (see Guiso et al. (2006) for a useful framework).⁸

Section 2 describes features of the refugee settlement program relevant for our analysis. Section 3 describes the data, our sample construction, and our proxies for financial literacy externalities. Section 3 presents the estimation model, while section 4 reports our findings. Section 5 concludes and presents policy implications for financial education programs and for placement of refugees. The Online Appendix contains variable definitions, descriptive statistics and more detailed presentation of estimates, robustness exercises, and some in-

⁶In principle, observation can lead to adjustments of financial behavior without information flows through the perception of norms, consistent with models of conformity (Bernheim, 1994). Social comparison considerations, such as "role model" effects, may also be present (Glaeser and Scheinkman, 2001; Akerlof, 1997).

⁷Hong et al. (2004) found that sociability, proxied by church attendance, participation in social clubs and similar activities, is related to greater tendency to hold stocks. Kaustia and Knuepfer (2012) found that the stock market performance of neighbors influences stock market entry. Georgarakos et al. (2014) found that those who perceive themselves as earning less than the average of their peers are more likely to borrow, to borrow larger amounts, and to worsen their indicators of potential financial distress.

⁸In a pioneering paper, Carroll et al. (1994) examined the role of culture for saving patterns at the individual level, while Guiso et al. (2006) looked at national saving rates. Osili and Paulson (2008) found a link between the degree of investor protection in the country of immigrant origin and the probability of the immigrant to participate in the stock market. Guiso et al. (2004) focused on use of basic financial instruments, such as writing a check or purchasing a share, and found that this is affected by the level of social capital. Guiso et al. (2006) provided evidence that trust is influenced by ethnic origin in US data, while Guiso et al. (2003) found evidence that trust is influenced by religion, both pointing to the relevance of culture. Haliassos et al. (2016) found that financial behavior differs across cultural groups of migrants, controlling for a range of characteristics, but these differences diminish with exposure to host country institutions.

formation on electoral districts.

2 Relevant features of the refugee placement policy

We exploit a rare natural experiment, a Swedish policy of exogenously allocating refugees to parishes and apartments. In 1985, the Swedish Immigration Board was assigned the task of placing refugees who moved to Sweden for reasons other than family reunification in particular apartments, in response to complaints from certain municipalities that they were bearing disproportionate burdens of absorbing immigrants in the 1980s. Almost all municipalities participated in the program, which went on until 1994 but with strictest implementation during the period 1987-91, on which we focus. The Swedish Immigration Board (quasi-) randomly assigned refugees to an initial parish and apartment within that parish.⁹

Our causal analysis relies on the assumption that, given the observed characteristics of the refugee, the characteristics of initial location are independent of unobserved individual characteristics determining the probability of outcomes we study (saving for retirement or holding stocks or other, labor-related choices discussed below) ten to twenty years later in life. The way in which the placement program assigned refugees to particular apartments is relevant for the validity of this assumption.¹⁰

Placement occurred shortly after the refugee obtained a residence permit. Given the motivating concern of allocating burden, the placement program did not assign refugees to apartments based on some lottery but mainly on the basis of availability of public housing (a suitable apartment). Program officers also took into account the education level of the refugee, whether others speaking the same language existed in the area of placement under consideration, and whether the refugee was married or single, with the latter being more difficult to place in view of limited availability of small apartments. Refugees were also asked to state their preferences. Importantly, there was no personal interview.

In our estimation, we are able to control for all relevant observable characteristics of the refugees that may have influenced their initial assignment. How could unobserved refugee characteristics enter the determination of initial placement? One channel might be provision of information to the placement officers outside what is recorded in the data.

⁹For details about the policy experiment, see Edin et al. (2003) pp. 333-335. Edin et al. (2003) and Åslund et al. (2011) use this natural experiment to study different issues, namely the consequences of living in enclaves for labor market outcomes and to what extent immigrant school performance is affected by the characteristics of neighborhoods in which they grew up, respectively.

¹⁰Our use of STATIV allows us to identify precisely the refugees among migrants to Sweden in the relevant period.

Since there was no interview and no further contact between the officers and the refugees, this channel can plausibly be ruled out.

Another possibility is that the statement of refugee preferences influenced placement, and these preferences were themselves influenced by unobserved factors also relevant for asset participation. Descriptions of the process and interviews with placement officers (Åslund et al., 2011) make it clear that the key limiting factor in placement was the availability of an apartment and not the preferences of refugees. Refugees tended to apply for placement in the largest and better known cities, but the economic boom meant that very few places were available there. Further evidence that refugee preferences hardly influenced the outcome is that the allocation of refugees through the program differed from the pre-existing endogenous allocation across the country, as well as from the allocation that was observed after sufficient time had elapsed for refugees to relocate on their own without paying short-term costs (such as being able to enrol in language classes). All these considerations support the assumption that the characteristics of initial location are independent of unobserved individual characteristics determining outcomes.

3 Data and Measurement of Externalities

3.1 Data and sample construction

We use the LINDA and STATIV databases from Statistics Sweden for the years 1987 to 2007 to identify refugee immigrants and their reasons for immigration, characteristics of the households in the neighborhood of each respondent, and household financial behavior.

LINDA consists of an annual cross-sectional sample of around 300,000 individuals, or approximately 3% of the entire Swedish population, and an annual immigration sample of around 200,000 individuals, or approximately 20% of all immigrants in Sweden. The data contain detailed and highly accurate information on financial and demographic characteristics of each sampled household as well as characteristics of their place of residence for the period from 1999 to 2007. This dataset is key to observing refugee financial behavior over the medium and longer runs.

We are also able to observe features both of the parish and the electoral district where the refugee was originally assigned. In 2000, there were 2,482 parishes. The median individual lived in a parish with 8,660 inhabitants, while the median refugee lived in a parish with 14,148 in 2000, suggesting more concentration in metropolitan areas. There are approximately 5,700 electoral districts in Sweden, typically with 1000 to 2000 people in an

electoral district.¹¹

The STATIV database contains the entire Swedish population and combines a large number of different variables from different registers in Sweden. We use the information from STATIV as a supplementary database to LINDA, as STATIV provides very detailed and rich information about immigrants. These include special coding for reasons for residence (e.g., refugee immigrant or labor immigrant) and the type of refugee immigrant.

When constructing the working sample, we adopt a conservative strategy in order to minimize potential misclassification or measurement errors. We restrict our attention to immigrants who entered Sweden between 1987 and 1991.¹² Unlike some previous work, we are able to identify refugees among immigrants with great accuracy through use of the STATIV data and include in the sample only those immigrants who were registered as refugees. We exclude from the sample those refugees who have been recorded as coming to Sweden for work reasons, family ties and other extensions, studies, other reasons, as well as refugees who are flagged as having enough living supplies. In other words, we only consider those refugees who are indicated as being in need of protection, or having been admitted for humanitarian reasons, i.e., those who find themselves in a particularly weak situation and present no doubt that they had to comply with the location instructions given by the immigration authorities.

We take further precautions in minimizing the probability of misclassifications. Specifically, to exclude family reunification cases from the analysis, we drop refugees who at the time of their first appearance in the LINDA dataset belong to a household with an adult (i.e., 18+) already residing in Sweden or holding a Swedish citizenship. Finally, we only keep those refugee immigrants who were first sampled in LINDA in the year of immigration or in the following year.

Out of this conservatively constructed sample, we drop households with missing information on the initial place of residence or the current place of residence (where by "current" is meant the 1999-2007 period) of the refugee, or the year of immigration, or the country of refugee origin. As we need to match refugees to their environment, we also exclude observations if there is missing information on the share of neighbors who have particular educational qualifications (described below) or who save for retirement.

Despite this conservative approach, we end up with 4,061 refugee immigrants in the final sample in any given year. Descriptive statistics for the pooled sample of 36,513 observations are presented in Table I. The breakdown of refugees by country of origin and by

¹¹Information is available at <http://www.scb.se/sv/Hitta-statistik/Regional-statistik-och-kartor/Statistikatlasen/Valen-2010-i-interaktiv-kartform/> See also Online Appendix D.

¹²See also Edin et al. (2003).

year of immigration is shown in Table O.A.1. Slightly more than a quarter of the refugees came from Iran, 13.22 percent from Chile, while Iraq and Lebanon have about 9 and 8 percent, respectively. As shown in Panel B, more than half the refugees in the sample entered Sweden in 1988 or 1989, while the rest entered in 1987 or 1990, with only a few entering in 1991.

3.2 Proxying for financial literacy externalities

There is no single way to define financial literacy, and different researchers and organizations have done so in different ways.¹³ The most widely adopted definition of financial literacy focuses on knowledge of basic financial concepts and familiarity with the economic environment. Our basic premise, following Bertrand et al. (2000), is that individuals have the greatest scope for interaction with people in their immediate environment, here being proxied by their electoral district. The potential of refugees for improving their own financial literacy through such interaction is assumed to be an increasing function of the (log) share of financially literate people living in their electoral district.

We take advantage of detailed available information on the level of educational attainment and content of education for people living in each electoral district to consider three alternative measures of financial literacy among neighbors in order to shed light on the nature of the effect. Our benchmark measure refers to the share of neighbors in the electoral district who have business/economics education and have attended college. To make sure that theoretical knowledge is combined with knowledge of Swedish institutions, we exclude from the set of relevant neighbors in the base runs migrants who have less than 20 years in Sweden.¹⁴ Our second measure of financial literacy is the share of electoral district neighbors who have a quantitative educational background, regardless of whether they have been trained in economics and finance. A quantitative background typically facilitates the processing of information relevant for financial behavior and could also yield useful externalities. Our third measure focuses on neighbors who provide an example that respondents can follow, whether or not respondents fully understand the information that led to this financial behavior. This measure focuses on the share of electoral district neighbors who contribute to private retirement saving plans. It is analogous in spirit to Duflo and Saez (2002), who considered participation in a retirement product among fellow librarians in a university library, but it allows us to consider the broader population and influence on a second financial product beyond the one held by neighbors.

¹³For an overview, see Lusardi (2008) and Lusardi and Mitchell (2007).

¹⁴In section 5.3 we also consider a broader set of relevant neighbors, which includes migrants who have spent between 10 and 20 years in Sweden.

We also explore different definitions of the relevant neighbor circle. In benchmark runs, we restrict our measure of financially literate neighbors to those who are native Swedes or immigrants with more than twenty years in Sweden. In further runs, we include all immigrants who have been at least ten years in Sweden. As we consider different groups, we essentially vary the likely intensity of interaction between refugees and their neighbors, while we ensure that neighbors have the necessary practical knowledge of the Swedish financial system by not considering immigrants with less than ten years of stay in the country.

4 The Estimation Model

We focus mainly on two aspects of financial behavior, participation in stocks and saving for retirement through private accounts. We explore further possible channels of influence and outcomes in Section 5.5 below. In modeling outcomes, we control for a wide range of observable household characteristics, introduce a number of fixed effects as well as random effects depending on the estimation method, and estimate the impact of measured exposure to financial literacy externalities in the initial neighborhood of assignment (electoral district).

In our benchmark regression (1), we estimate the long shadow of financial literacy in the original electoral district on refugee financial behavior over the medium and the longer run, controlling for unobserved characteristics of the parish that contains that electoral district. We estimate a model of the following form:

$$Y_{iklj0t} = \alpha \cdot X_{it} + \beta \cdot FLShare_{l0} + \gamma_j + \gamma_k + \gamma_0 + \gamma_t + \epsilon_{iklj0t} \quad (1)$$

where Y_{iklj0t} refers to the relevant aspect of financial behavior of household i from country of origin k that arrived in year $0 \in \{1987, 1988, 1989, 1990, 1991\}$, was initially placed in electoral district l and parish j and is observed in period t . $FLShare$ is the share of financially literate neighbors in the household's initial electoral district, l , in the year of arrival, 0. For our medium run analysis, the observation years are $t = 1999, \dots, 2003$, while for the longer-run analysis, the corresponding years are $t = 2004, \dots, 2007$.

We are able to control for a wide array of observable household characteristics, denoted by X_{it} . These include disposable labor income (in logs), age categories, gender, occupational status (unemployed, retired, employed, student), marital status, number of adults in the household, number of children in the household, educational attainment (less than high school, high school and college graduate), position of the household in the distribution of net wealth (except that, when we consider stocks, we exclude the asset class in question

from the computation of net wealth), and working in the financial sector or working for the government, all measured in the year of observation of financial behavior, t .

The coefficient of interest is that on the share of financially literate neighbors. As placement in the initial electoral district is exogenous to the refugee, we do not use instrumental variable estimation but can use OLS or probit estimators instead. Such estimation allows financial literacy in the initial location to influence subsequent financial behavior through various channels other than those for which we explicitly control. We exploit the panel nature of the dataset to account for a number of fixed effects, as well as for household unobserved heterogeneity. We introduce fixed effects for the parish of initial placement, γ_j ; the country of origin, γ_k , the year of arrival, γ_0 , and the year of observation, γ_t . We also correct the standard errors by clustering at the initial electoral district level.

Parish fixed effects should control for the possibility that a positive coefficient on the externalities variable reflects, at least in part, exposure to common factors in the greater neighborhood. For example, a higher share of financially literate neighbors in the electoral district may reflect characteristics of the initial parish (e.g., a larger number of brokers and insurance agents, banks or other financial institutions, its urban rather than rural nature, or greater neighborhood ambience that attracts sophisticated individuals to the broader area of the parish), and these unobserved parish-specific factors tend to generate both a more knowledgeable base of neighbors in the electoral district and better financial outcomes for the refugees in that district. This is an instance of 'correlated effects', where the parish environment influences positively both the quality of (electoral-district) neighbors and refugee financial behavior, without a direct link between the two. Initial parish fixed effects are identified, both because the arrival year of refugees to that initial parish is not the same, and because the parish typically includes more than one electoral districts. Both factors create variation in the initial share of financially literate (electoral-district) neighbors for refugees at the same initial parish.

5 Externalities from Financially Literate Neighbors

We begin our analysis by focusing on causal effects of exposure to neighbors with college education and a business or economics background on refugee financial behavior. We estimate the effect of the share observed in the initial electoral district of exogenous placement by the immigration authorities, controlling for refugee characteristics, some of which might have influenced that placement, as well as for supply-side factors in the greater area of the parish, macroeconomic and other year-specific factors in the year of arrival and in that of observation, and considerations that might be specific to refugees from the particular coun-

try of origin. We consider behavior over different runs, the nature of operative links, and the role of the likely intensity of interaction. We present results from linear probability models in the main text. In order to keep tables in the main text to a minimum, we relegate some tables as well as more complete versions of other tables to Online Appendix B. Online Appendix C presents probit estimates of average marginal effects that correspond to the estimates from linear probability models presented in the main text. Tables in the main text are indicated by roman numerals, and those in the online appendix by the prefix O.A. followed by a number.

5.1 Presence and time dimension of effects

Table II presents the estimation results for the full set of years during which financial behavior is observed, 1999-2007, before we distinguish between the "medium run" effects on financial behavior in the period 1999-2003, and the "longer run" effects in the period 2004-2007. Columns (iii) and (iv) differ from (i) and (ii) in that they also control for unobserved household heterogeneity through random effects. We see that when the period is taken as a whole, the share of neighbors who had attended college and had economics or business education in the initial electoral district of placement has a positive effect both on the tendency to save for retirement and on the tendency to hold stocks. This positive effect is present, controlling for a wide range of household characteristics as well as for the fixed effects described above, which include unobserved relevant features of the parish of initial placement.

Regardless of whether we control for household heterogeneity through random effects estimation or not, we find somewhat larger estimated effects and greater statistical significance for the probability of holding stocks than for the probability of saving for retirement. This is consistent with the idea that stock investment is more involved, because of its informational intensity and its riskiness, compared to saving for retirement. In such a case, respondents are more likely to benefit from knowledge transfers to them from the environment.

These results are consistent with initial placement casting a long shadow on the evolution of subsequent financial behavior of the refugee, even controlling for how the refugee's income, wealth, education, marital status and household size develop in the host country. This suggests that the quality of the initial placement of refugees (in our case, with regard to financial literacy of neighbors) matters for subsequent financial behavior, roughly between 10 and 20 years after the time of entry.

The sign and statistical significance of other controls is mostly consistent with what has been found in household finance regressions for these instruments to date. It is noteworthy

that educational attainment of the household head continues to be statistically significant and to correlate with investment in stocks and saving through private retirement accounts even when the role of a financially literate neighborhood is acknowledged. On the other hand, having a household head that works in the financial sector ceases to be significant for good financial outcomes when financial literacy externalities in the neighborhood are included in the regression. What seems to matter is exposure to financially literate neighbors rather than working in the financial sector per se. Having a larger number of children is negatively associated with saving for retirement, although estimates are not consistently significant. The sign of the relationship of the number of children to the tendency to hold stocks switches once we allow for unobserved heterogeneity through random effects.¹⁵ The presence of children might also encourage social interactions, but we have not found significant interaction effects strengthening financial literacy externalities in unreported regressions.

Tables O.A.2 and O.A.3 distinguish between effects of financial literacy externalities in the initial neighborhood over the medium run (1999-2003) and over the longer run (2004-2007), respectively. Separating the two "runs" allows not only the effect of financial literacy externalities but also the relationship of participation probability to household characteristics and other factors to differ across the two periods of observation of financial behavior, rather than forcing them to be the same.

An interesting pattern emerges. When considering only the medium run from the initial placement (Table O.A.2), we do find positive coefficient estimates for participation in both assets, and somewhat higher ones when we control for unobserved heterogeneity through random effects. However, these effects of initial exposure to financially literate neighbors are only statistically significant (at the 10 percent level) for the simpler of the two assets, namely retirement saving, and not for the riskier and more informationally intensive stocks. When we consider the longer run effects of financial literacy externalities (Table O.A.3), we find strongly statistically significant and quite sizeable effects on participation in stocks, but no remaining significant effects on retirement saving. Controlling for unobserved heterogeneity, a 10 percent increase in the share of neighbors with economics or business education in the electoral district of initial placement is estimated to add 10.4 percentage points

¹⁵The estimated sign is typically negative or insignificant in cross-sectional household finance regressions that do not allow for financial literacy externalities. Here we obtain a significant positive relationship when we allow for unobserved heterogeneity through random effects. These sign reversals highlight the multidimensional role that the number of children plays in the decision to participate in the stock market. On the one hand, for given resources, a larger number of children implies larger costs and greater committed expenditures that leave fewer resources for stockholding. On the other hand, a larger number of children may also provide a greater impetus to seek the wealth-generating potential of the equity premium and a more diversified safety net to parents facing stockholding risks.

to the probability that the household participates in the stock market over a horizon of 15 to 20 years. Interestingly, the pattern of signs and significance of the other household controls remains the same as in the regression spanning all years of observed financial behavior.¹⁶

We will further examine this change in relevance of initial exposure to financial literacy externalities between the medium and the longer run below. The difference we found between effects on medium- and on longer-run behavior, however, is consistent with financial literacy externalities being more relevant for the riskier and more informationally intensive asset but also with a longer "gestation period" during which information is absorbed and the idea of stockholding gradually matures.

5.2 Exploring the links: education, gender, and content

In this section, we try to shed light on the underlying mechanism by investigating the strength of financial literacy externalities for different subsamples. We consider two important sample splits, by education and by gender, and we compare the coefficients on the exposure to financially literate neighbors across each pair of subsamples.¹⁷

Table IV shows that the effect of financial literacy externalities is present only for the more educated subsample, namely those whose heads have high school education or more, but not for those with less than high school education. This is true regardless of whether we examine medium- or longer-run effects. Interestingly, we find that refugees with high-school education or more benefit from financially literate neighbors in both asset participation decisions, retirement saving and stockholding, but the effect on stock market participation increases in estimated size and significance level as we move from the medium to the longer run. The estimated effect on retirement saving remains roughly the same, and it is only significant at the 10 percent level in the longer run.

The nature of the allocation process performed by immigration officials, focused as it was on education, language, and family size, could a priori result in differences across subsamples in exposure to externalities, and these might confound results on the operativeness of different channels per se.¹⁸ Table O.A.4 verifies that allocation of refugees by immigration officials did not result in different exposures of the two subsamples to financially literate neighbors, regardless of the financial literacy measure used. The two subsamples

¹⁶Table O.A.25 presents average marginal effects for the medium, the longer run, and the full period of observation of financial behavior using probit estimation. We see that these estimates of average marginal effects are very close numerically to the corresponding estimates from the linear probability model and with the same pattern of statistical significance, confirming robustness to the estimation method used.

¹⁷Obviously, by splitting the sample and carrying out separate estimations, we also allow the relationship of other factors to the probability of participation to differ across subsamples.

¹⁸See Edin et al. (2003) on the governing criteria for allocation of refugees to apartments.

exhibit comparable average exposure to financially literate neighbors, as well as comparable variation in this exposure. Given the absence of sorting, our findings on the education split suggest that higher educational attainment is crucial if people are to benefit from financially literate neighbors. Higher educational attainment normally provides people with increased ability to ask the right questions and evaluate the information they obtain from others. More educated people also tend to be more likely to become interested in acquiring such information, as stocks and private retirement plans are more salient to them in the course of their daily occupation.

Are financial literacy externalities important in influencing participation behavior of both female- and male-headed households? The question does not have an a priori obvious answer in light of existing research in household finance. On the one hand, existing literature on financial literacy draws attention to more limited financial literacy among women and lower self-confidence in dealing with finances. These are evidenced by lower scores of women in financial literacy tests, and by greater tendency to opt for the "Don't Know" answer and to give a wrong answer if they attempt the question (Lusardi and Mitchell, 2014; Bucher-Koenen et al., 2016). On the other hand, there is considerable evidence that males are more likely to exhibit overconfidence with respect to stock trading (Barber and Odean, 2001) and to be less willing to get or accept financial advice (Hackethal et al., 2012). In principle, overconfidence and more limited willingness of males to consult with others may limit the scope for financial literacy externalities to affect financial behavior of males, so that such externalities can contribute to mitigating the gender gap found in the literature so far. However, this is not what we find.

When we split the refugee sample by gender (Table V), we find that financial literacy externalities are operative for households headed by males but fail to have statistically significant effects on participation of females. Female-headed households who find themselves in a neighborhood with a larger share of economics or business-educated neighbors are not systematically influenced by them in their saving for retirement or stock market participation decisions, either over the medium- or over the longer run. Male-headed households, on the other hand, are more likely to participate in either asset over the medium run when they have been exposed to a greater share of financially literate neighbors, and the effect persists for stocks and becomes larger over the longer run. Thus, instead of financial literacy externalities providing a mitigating mechanism, they can actually contribute to the gender disparity in asset market participation. In addition to verifying that there was no sorting by immigration officials (see Table O.A.4), we have explored the possibility that the result is due to a tendency of women to talk more to women. In unreported regressions, we find no effect even when we restrict attention to the share of financially educated women in

the initial parish.

Our findings on the education and gender sample splits are consistent with the view that financial literacy of neighbors influences household choices through transfer of knowledge and information that needs to be received, processed, understood, and acted upon by the household in question. In view of existing literature on participation, our findings point to the conclusion that ability to process new financial information from neighbors and self-confidence to act upon it, both of which have been found to be larger for more educated and for male-headed households, tend to overcome relative unwillingness to consult others because of overconfidence, thus making financial literacy externalities operative.

Further insights into the channel of transmission can be obtained by varying the potential content of financial literacy externalities. Instead of considering neighbors with business or economics education as potential sources of such externalities, we next consider a broader set that includes all neighbors with quantitative degrees. Summary results for the full observation sample, the medium run, and the longer run are reported in Table VI, while full results are reported in Tables O.A.5, O.A.6, and O.A.7, respectively.

When considering this broader group of neighbors, with ability to process quantitative information but no specialized knowledge of economics or business, we find smaller corresponding estimated effects of financial literacy among neighbors, regardless of whether we focus on the medium or the longer run effects on financial behavior. We confirm the pattern of significant effects on retirement saving over the medium run and on stockholding over the longer run, but with a smaller size.¹⁹ Moreover, the pattern of results for the sample splits based on education and gender of the head remains generally the same as for the case of neighbors with economics or business education (see Tables O.A.8 and O.A.9).²⁰

Our findings with this broader notion of financial literacy among neighbors suggest that financial literacy externalities from neighbors with college-level knowledge of economics or business tend to be larger than those generated by neighbors who have developed capacity for processing quantitative information in general but may not have specialized knowledge relevant for financial behavior. This finding reinforces the view that the process through which financial literacy externalities operate is not mindless imitation but one in which content and knowledge matter.

¹⁹Taking the observation period as a whole, the regression for the full sample shows very comparable effects of quantitatively educated neighbors on retirement saving and on stockholding participation.’

²⁰The exception is that, unlike for neighbors with business or economics education, quantitatively educated neighbors influence saving for retirement decisions of females rather than of males in the medium run.

5.3 Salience and intensity of interactions

Benchmark estimation so far has proceeded by including in the set of relevant neighbors those who are Swedes or immigrants with more than 20 years of stay in Sweden. In this section, we broaden the neighbor circle over which we measure financial literacy to include also more recent migrants that have stayed in Sweden between 10 and 20 years, and we examine the estimated size and significance of the coefficient on the proxy for financial literacy externalities computed for the new circle under consideration. By expanding the relevant social circle to include more recent immigrants, we increase the probability that the refugee respondent interacts with the neighbors of a given level of financial literacy. Such increase could be a result of simply including more neighbors in the pool, but it can also be reinforced by the likely greater tendency of recent migrants to associate with refugees sharing similar levels of assimilation to the local culture.²¹

Table O.A.10 presents summary results, while full estimation results are presented in Tables O.A.11, O.A.12, and O.A.13. We find that a given increase in the share of neighbors with college-level economics or business education among this expanded circle of neighbors results in greater estimated increases in the probability of participation in retirement saving, which are even significant for longer-run behavior, but in smaller estimated increases in stock market participation (with similar patterns of significance as in the benchmark). This combination of greater intensity of the effect on retirement saving and reduced intensity of the effect on stockholding under the expanded circle of neighbors is remarkably robust to undertaking sample splits by education and gender and to considering quantitative education as the basis for defining the share of financially literate neighbors.²²

This robust mixed result is intriguing. If greater intensity of interaction with the relevant circle of neighbors were the full explanation, we would expect to observe higher estimated effects on participation in both financial instruments (retirement accounts and stocks). However, we observe that the estimated effect of financial literacy externalities on stocks is now smaller.

This suggests the presence of a second, offsetting factor, and results in earlier work on the Swedish native and migrant population provide an important clue. Haliassos et al. (2016) have shown that, as migrants spend time in the host country, they progressively raise their stock market participation probability to approach that of Swedes, controlling for household characteristics. There is considerable such adjustment between the tenth and the twentieth year of stay in Sweden, and this is the relevant period for the addition to the

²¹An alternative in this context might be to consider neighbors from the same country. This is not feasible, however, due to the small numbers involved.

²²Full results are presented in Tables O.A.14 to O.A.18.

set of neighbors considered in this section. Thus, a plausible second factor that produces the particular pattern of results for stockholding is the changing salience of stocks, controlling for age and other relevant migrant characteristics, as a function of the time the migrant neighbor has spent in the host country. The expanded set of migrant neighbors we consider have significantly lower stock market participation rates than those more closely assimilated to Swedes, and are therefore less likely to find stocks salient and convey information on stockholding than the benchmark circle of neighbors does.

5.4 Financial choices of neighbors

We have focused so far on a key characteristic of neighbors, educational attainment and content, and its tendency to encourage participation in financial instruments. Existing literature on financial literacy has provided considerable evidence that those who are more financially literate are also more likely to participate in financial products, such as retirement saving (Lusardi and Mitchell, 2007) or stockholding (van Rooij et al., 2011). The separate literature studying peer effects on financial behavior has delivered strong evidence that the tendency of peers to hold a particular retirement product or to be successful in stockholding tends to make others in the peer group more likely to participate in retirement products (Duflo and Saez, 2002) or in stocks (Kaustia and Knuepfer, 2012), respectively. Combining these results in our context of financial literacy externalities raises the possibility that the key channel through which such externalities operate is emulation of financial choices of neighbors with no significant role for transfer of knowledge useful for financial decisions. If this is so, then promoting participation in financial products may be left to marketing campaigns without any need for financial education initiatives. In this section, we investigate this possibility.

The literature on peer effects has recognized the potential presence of both types of influence, those arising from underlying characteristics of the peers (termed by Manski “exogenous” or “contextual” effects) and those resulting from emulation of financial choices of peers (termed “endogenous” effects). In their seminal study, Duflo and Saez (2002) investigated whether individuals are directly influenced by the financial choices of their peers, in a setup where participants and their peers were quite homogeneous in characteristics, and participation in only one (retirement) product was studied.²³ In our data, we observe participation of electoral-district neighbors in retirement saving, their heterogeneous financial literacy according to the two criteria already examined, as well as participation of the

²³Duflo and Saez (2002) used data on participation of librarians in each library of a large University in a particular retirement product. While this helped to isolate the effect of peers’ use of the retirement product, it provided little variation in educational characteristics among peers and no other asset to consider.

respondents (refugees) in two financial instruments, one of which (stockholding) is distinct from the peer instrument observed (retirement saving).

Table VII presents summary results on the influence of the share of neighbors who were saving for retirement in the initial electoral district of placement on the subsequent tendency of refugees to participate in retirement saving or in stockholding over different time horizons.²⁴ We see that greater presence of retirement savers in the district of initial placement has statistically significant positive effects on refugee participation in both instruments over the longer run, but not in the medium run (where effects are not significant). The estimated size of these effects is generally smaller than the size we estimated for the share of neighbors with economics or business background (Tables II to O.A.3) and for the share with quantitative background (Table VI). Moreover, the effects of retirement savers in the initial neighborhood take longer to be visible, as they are not significant over the medium run but only in the longer run.²⁵ This comparison implies that a greater share of peers with knowledge relevant for financial decisions had bigger and more immediate effects on refugee financial behavior than did a comparable percentage increase in the share of peers participating in retirement accounts.²⁶

These results support further the hypothesis that financial literacy externalities go beyond imitation and involve the transfer of relevant knowledge, reinforcing other types of evidence presented in our findings above. Additional evidence for this view comes from considering effects on refugee participation in two assets. If the effect of participating in retirement accounts were purely one of imitation, we would expect that it would only or mainly be visible for the asset held by the neighbors. What we find, however, is that it extends to the other asset (stocks), and it is actually estimated to be larger for refugee participation in this more information-intensive asset than for the retirement asset.

Tables O.A.23 and O.A.24 present results when we broaden the neighborhood circle to include migrants with between 10 and 20 years in Sweden. We see that the estimated effects of having neighbors who save for retirement in the initial district of exogenous placement are actually estimated to be larger than those for the benchmark circle of relevant neigh-

²⁴Full estimation results for retirement saving by refugees are in Table O.A.19 and for stockholding in Table O.A.20.

²⁵In unreported regressions, we have also included the share of retirement savers together with either the share of neighbors with business or economics education, or the share of those with quantitative education. We found no case (whether for the full sample, the medium run, or longer run behavior) in which the share of retirement savers in the electoral district was significant but the corresponding education share was rendered insignificant.

²⁶When we split the sample by education, we find a statistically significant effect on retirement account participation already in the medium run for the more educated (Table O.A.21). Results on the gender split are mixed, with short-run effects on retirement saving appearing for women and longer-run effects on stockholding only for men (Table O.A.22).

bors, and even to turn significant in the medium run in the case of retirement saving. These findings are quite intuitive in the context of intensity of interactions and of salience. The expansion of the relevant circle of neighbors to include more recent migrants with retirement accounts increases the likely intensity of interactions. Seeing a larger share of retirement savers in this expanded circle signals to the refugees that retirement saving is likely more relevant for them. Interestingly, when we control for unobserved heterogeneity through random effects, the estimated effect on stockholding is larger than that on participation in retirement saving, as for the smaller circle of neighbors described above.

5.5 Other channels of influence: employment and location

Our analysis so far has focused on the influence of financial literacy externalities on stockholding and on retirement saving, controlling for attributes related to income and employment. In this section, we investigate whether there are important labor market and locational channels of influence that we have so far underplayed. Tables VIII and O.A.27 present effects of the initial share of financially literate neighbors (proxied by business or economics education and by quantitative education, respectively) on labor-market outcomes and on eventual location of the refugees (by the year 1999). We consider three labor-market outcomes: whether the respondent ends up working in the financial sector, the level of earnings attained,²⁷ and whether the respondent is unemployed.

As the Tables show, we find no evidence of an effect of the initial share of financially literate neighbors on the level of earnings and on the tendency to be unemployed, either in the medium or in the longer run and regardless of the definition of financial literacy employed. We only find an effect on the probability that the refugee ends up working in the financial sector over the longer run.

This pattern of results is consistent with the view that financial literacy externalities are unlikely to operate through labor market channels; and when they do, their influence is mainly in the form of encouraging people to acquire and use financial expertise. In turn, this reinforces our conclusion above that financially literate neighbors communicate useful information to the refugees rather than encouraging mindless imitations.²⁸

Finally, it is also useful to ask whether financially literate neighbors in the initial lo-

²⁷We report results using the broad income definition that includes labor income, income from entrepreneurship, and employment related transfers (see also Edin et al. (2003) and Åslund et al. (2011)) and consider only people with positive earnings, as is standard in the labor literature. These results are robust to using different earnings definitions, and defining the income at the household or individual level.

²⁸A more mechanical link, in the form of financially literate neighbors simply providing professional connections to refugees so that they get a job in the financial sector, is hard to reconcile with the absence of any effect on the level of earnings and on the probability of unemployment.

cation influence subsequent financial behavior mainly by affecting the probability that the refugee eventually moves to another location. As results from the cross-sectional regression in the final column indicate, we find no evidence that the initial share of financially literate neighbors influences the location of the refugee by year 1999.

All in all, findings in this section support the view that the main channel through which financial literacy externalities influence financial behavior over the medium and longer term is the provision of financial information and transfer of expertise rather than indirect channels having to do with improved employment or locational prospects.

6 Conclusions and Policy Implications

This paper uses unique administrative data on refugees to Sweden and a quasi-field experiment of exogenous allocation of refugees in order to estimate the effect of access to financially literate neighbors on two important aspects of household financial behavior, namely saving for retirement and participation in stockholding. As we can track refugee households over twenty years, we are able to estimate the effects of the exogenous component of exposure to financial literacy externalities over the medium run (ten to fifteen years after initial placement) and the longer run (about fifteen to twenty years). The nature of the experiment and of the data allow us to address thorny causality issues related to "correlated effects" arising from endogenous choice of neighborhood. We focus on the influence of financial literacy of neighbors on respondent financial behavior, which can be central for assessing social multipliers of programs that are effective in raising financial literacy in segments of the population. We also explore whether such influence could operate through other channels related to the labor market.

We find evidence of statistically and economically significant effects of financial literacy in the exogenous initial location of refugees on their financial behavior in the medium and in the longer run, controlling for unobserved features of their location and for a number of individual characteristics. Effects on the act of saving for retirement tend to be significant in the medium run, while effects on stockholding are significant in the longer run.

We next turn to exploring the nature of financial literacy externalities and channels through which they operate. We find evidence that the effects are operative for the subsample of refugees who have at least a high school degree, but not for those with less than high school education. We also find that the effect tends to operate through male- rather than female-headed households. We confirm that these results are not plausibly due to sorting of more educated and male refugees to areas with greater financial literacy nor to the choice of financial literacy concept.

When we vary the definition of financially literate neighbors to include all those with quantitative education, we find the same pattern but a smaller estimated size of effects across all runs and sample splits. We next expand the circle of relevant neighbors to include also migrants who have spent between 10 and 20 years in Sweden and are more likely to be interacting with the refugees. We find that the estimated size of financial literacy externalities on participation in retirement saving increases, while that on stock market participation decreases. We cite evidence that the more recent migrants are less likely to participate in stocks, conditional on their characteristics, and thus less likely to discuss them. The conflict between stronger interaction - which should strengthen effects on both assets - and greater salience of stocks is consistent with the opposite effects we observe.

We also consider the possibility that financial literacy externalities operate mainly through imitation of financially literate neighbors. We do find that the initial share of neighbors participating in retirement saving has a longer-run effect on asset participation of refugees. The effect is, however, smaller than for the share of financially literate neighbors under either definition, and operative only in the longer run. Moreover, contrary to what one expects in case of pure imitation, the effect of observing neighbors with retirement saving extends to refugee longer-run participation in both assets, and it is actually more pronounced for the other asset (stocks). When we expand the circle of neighbors to include more recent migrants, effects of having more retirement savers in the initial neighborhood increase, as would be expected both from greater likely intensity of interaction between refugees and their neighbors and from greater similarity in the financial products that both find salient.

Finally, when we consider other possible channels of influence, we find no evidence that the effects operate through employment or locational prospects, except in encouraging financial sector placement. All in all, our findings suggest the presence of significant financial literacy externalities that extend beyond imitation to the transfer of knowledge relevant for financial behavior, and that are increasing in economics and business content.

These findings contribute to the long-time search for convincing evidence of a causal impact of financial literacy on economic behavior and outcomes, and to policy discussions on the appeal of financial education programs. While financial literacy externalities are likely to lower the cost of effective financial education initiatives for given effects on financial literacy, their uneven impact is likely to widen disparities in financial literacy that should not be ignored in policy design. To the extent that externalities are operative only for people who have the educational background and confidence to receive and process relevant financial knowledge, they can widen the observed gap in financial behavior and outcomes.

The focus on refugees, albeit for econometric reasons, provides also some conclusions in a very different context, relevant for the ongoing refugee crisis. Our results highlight the

medium and longer-term importance of placing refugees in neighborhoods where they can benefit from the knowledge and (financial) literacy of their neighbors. The finding that it is the more educated and financially confident refugees that are likely to benefit from financial literacy externalities does not suggest that including such background considerations in refugee placement is likely to meet with objections from highly educated local communities. Our findings also suggest that the size of financial literacy externalities does not depend only on the share of financially literate neighbors but also on the preconditions for intensive interactions with those neighbors. Placing more educated refugees in more educated neighborhoods and promoting interactions with their new neighbors significantly enhances their tendency to save for retirement and to engage in active participation in information-intensive financial products.

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Table I: Summary Statistics

	<i>Full Sample</i>			<i>Medium-Term</i>			<i>Longer-Term</i>		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
<i>Panel A: Dependent Variables</i>									
Saving for Retirement	36,513	0.26	0.44	20,303	0.23	0.42	16,210	0.30	0.46
Stockholding	36,513	0.37	0.48	20,303	0.37	0.48	16,210	0.36	0.48
<i>Panel B: Financial Literacy Externalities (at initial Placement)</i>									
Share of neighbors with economics/business education	36,513	0.02	0.03	20,303	0.02	0.03	16,210	0.02	0.03
Share of neighbors with quantitative education	36,513	0.05	0.05	20,303	0.05	0.05	16,210	0.05	0.05
Share of neighbors who save for retirement	36,513	0.19	0.10	20,303	0.19	0.10	16,210	0.19	0.10
<i>Panel C: Household Controls</i>									
Log disposable Income	36,513	12.99	0.57	20,303	12.89	0.56	16,210	13.11	0.57
Age 30-45	36,513	0.51	0.50	20,303	0.58	0.49	16,210	0.41	0.49
Age 45-60	36,513	0.39	0.49	20,303	0.32	0.47	16,210	0.49	0.50
Age 60-75	36,513	0.07	0.26	20,303	0.05	0.22	16,210	0.09	0.29
Male	36,513	0.67	0.47	20,303	0.67	0.47	16,210	0.67	0.47
Unemployed	36,513	0.32	0.47	20,303	0.35	0.48	16,210	0.29	0.45
Retired	36,513	0.09	0.29	20,303	0.09	0.28	16,210	0.10	0.30
Employee	36,513	0.56	0.50	20,303	0.52	0.50	16,210	0.60	0.49
Married	36,513	0.60	0.49	20,303	0.59	0.49	16,210	0.60	0.49
Number of Adults	36,513	1.96	0.95	20,303	1.89	0.91	16,210	2.05	1.00
Number of Children	36,513	1.01	1.27	20,303	1.10	1.31	16,210	0.91	1.22
High School Graduate	36,513	0.41	0.49	20,303	0.41	0.49	16,210	0.42	0.49
College Graduate	36,513	0.31	0.46	20,303	0.30	0.46	16,210	0.32	0.47
Working in the Financial Sector	36,513	0.00	0.05	20,303	0.00	0.05	16,210	0.00	0.05
Working for the Government	36,513	0.20	0.40	20,303	0.18	0.38	16,210	0.22	0.42

Note: This table presents descriptive statistics for the variables employed in the empirical analysis. The sample is a balanced sample of 4,061 refugee immigrants. The medium-term refers to the time period from 1999 to 2003, and the longer-term refers to the period from 2004 to 2007, respectively. The mean and standard deviation are calculated on the full pooled sample. The monetary variables are defined in SEK. For variable definitions, see Online Appendix A. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table II: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Full Observation Period (1999-2007)

	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
	(i)	(ii)	(iii)	(iv)
Fin Lit Share	0.41405* (0.2442)	0.61315** (0.2648)	0.47840* (0.2560)	0.70443** (0.2792)
Income	0.18013*** (0.0127)	0.20321*** (0.0123)	0.08000*** (0.0085)	0.08100*** (0.0076)
Age 30-45	0.03493* (0.0181)	-0.02578 (0.0240)	0.04547*** (0.0164)	0.00303 (0.0180)
Age 45-60	0.06202*** (0.0214)	-0.05972** (0.0266)	0.06963*** (0.0181)	-0.00004 (0.0198)
Age 60-75	-0.04522* (0.0257)	-0.10805*** (0.0324)	-0.01859 (0.0233)	-0.02188 (0.0253)
Male	-0.04132*** (0.0128)	-0.05365*** (0.0133)	-0.03309*** (0.0127)	-0.03189** (0.0134)
Unemployed	-0.01428 (0.0174)	-0.01542 (0.0197)	-0.00220 (0.0119)	-0.00675 (0.0114)
Retired	-0.03709 (0.0230)	-0.06787*** (0.0247)	-0.01561 (0.0152)	-0.04130*** (0.0146)
Employee	0.04786** (0.0190)	0.05625*** (0.0210)	0.03096** (0.0132)	0.02016 (0.0123)
Married	0.01461 (0.0127)	0.02582* (0.0134)	0.00835 (0.0086)	0.00863 (0.0093)
Nbr of adults	-0.02040*** (0.0070)	0.00005 (0.0069)	0.01402*** (0.0048)	0.03995*** (0.0050)
Nbr of children	-0.02247*** (0.0048)	-0.00448 (0.0050)	-0.00372 (0.0038)	0.02418*** (0.0039)
High school Dummy	0.04946*** (0.0131)	0.06410*** (0.0140)	0.04123*** (0.0126)	0.05155*** (0.0115)
College and more Dummy	0.09739*** (0.0153)	0.16755*** (0.0175)	0.07744*** (0.0159)	0.15253*** (0.0169)
Net wealth quartile II	-0.01189 (0.0104)	-0.02038* (0.0119)	-0.00462 (0.0069)	0.00183 (0.0071)
Net wealth quartile III	-0.00281 (0.0113)	-0.02138 (0.0131)	0.01087 (0.0077)	-0.00662 (0.0079)
Net wealth quartile IV	0.10306*** (0.0144)	0.13910*** (0.0140)	0.03476*** (0.0092)	0.02258** (0.0088)
Financial sector Dummy	0.04859 (0.0893)	-0.05159 (0.0854)	0.00212 (0.0743)	0.03764 (0.0535)
Government sector Dummy	0.00639 (0.0136)	-0.04285*** (0.0144)	-0.01385 (0.0095)	-0.01292 (0.0091)
<i>Observations</i>	36513	36513	36513	36513
<i>Clustering</i>	<i>Electoral District</i>	<i>Electoral District</i>	<i>Electoral District</i>	<i>Electoral District</i>
<i>Time Fixed Effects</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>Country-of-Origin Fixed Effects</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>Arrival-year Fixed Effects</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>Unobserved HH Heterogeneity</i>	<i>No</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
<i>Neighborhood Fixed Effects</i>	<i>Parish</i>	<i>Parish</i>	<i>Parish</i>	<i>Parish</i>

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect). In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. In specifications (iii) and (iv), we control for unobserved household heterogeneity using a random effects estimator. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics education and at least some college attendance. We consider a balanced sample of 4,061 refugee immigrants and financial behavior in the period 1999-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table III: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term versus Longer-Term

	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
<i>Panel A: Medium-Term</i>	(i)	(ii)	(iii)	(iv)
Initial Fin Lit Ext	0.44802* (0.2526)	0.35481 (0.2801)	0.50404* (0.2613)	0.43628 (0.2928)
<i>Observations</i>	20303	20303	20303	20303
<i>Panel B: Longer-Term</i>	(i)	(ii)	(iii)	(iv)
Initial Fin Lit Ext	0.39095 (0.2798)	0.93888*** (0.2833)	0.44506 (0.2932)	1.03961*** (0.2935)
<i>Observations</i>	16210	16210	16210	16210
<i>Household Controls</i>	Yes	Yes	Yes	Yes
<i>Clustering</i>	<i>Electoral District</i>	<i>Electoral District</i>	<i>Electoral District</i>	<i>Electoral District</i>
<i>Time Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Country-of-Origin Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Arrival-year Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Unobserved HH Heterogeneity</i>	No	No	Yes	Yes
<i>Neighborhood Fixed Effects</i>	<i>Parish</i>	<i>Parish</i>	<i>Parish</i>	<i>Parish</i>

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect). In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. In specifications (iii) and (iv), we control for unobserved household heterogeneity using a random effects estimator. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics education and at least some college attendance. We consider a balanced sample of 4,061 refugee immigrants. Medium-term effects refer to financial behavior in the period 1999-2003, while longer-term effects refer to 2003-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table IV: Sample Split By Education: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term and Longer-Term

	High school and more		Less than high school	
	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
Panel A: Medium-Term	(i)	(ii)	(iii)	(iv)
Fin Lit Share	0.78922** (0.3346)	0.80131** (0.3570)	0.08768 (0.4102)	-0.26196 (0.4296)
Observations	14392	14392	5911	5911
Panel B: Longer-Term	(i)	(ii)	(iii)	(iv)
Fin Lit Share	0.70775* (0.3766)	1.42920*** (0.3521)	-0.22101 (0.5802)	0.29275 (0.4877)
Observations	11936	11936	4274	4274
<i>Household Controls</i>	Yes	Yes	Yes	Yes
<i>Clustering</i>	Electoral District	Electoral District	Electoral District	Electoral District
<i>Time Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Country-of-Origin Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Arrival-year Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Unobserved HH Heterogeneity</i>	Yes	Yes	Yes	Yes
<i>Neighborhood Fixed Effects</i>	Parish	Parish	Parish	Parish

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect) for two subsamples based on educational attainment. In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. In specifications (iii) and (iv), we control for unobserved household heterogeneity using a random effects estimator. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics education and at least some college attendance. We consider a balanced sample of 4,061 refugee immigrants. Medium-term effects refer to financial behavior in the period 1999-2003, while longer-term effects refer to 2003-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table V: Sample Split By Gender: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance: Medium-Term and Longer-Term

	Female		Male	
	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
Panel A: Medium-Term	(i)	(ii)	(iii)	(iv)
Fin Lit Share	0.19307 (0.4782)	-0.32645 (0.5073)	0.54860* (0.3226)	0.80739** (0.3448)
Observations	6620	6620	13683	13683
Panel B: Longer-Term	(i)	(ii)	(iii)	(iv)
Fin Lit Share	0.10904 (0.5189)	0.26643 (0.4946)	0.50389 (0.3762)	1.37665*** (0.3672)
Observations	5285	5285	10925	10925
Household Controls	Yes	Yes	Yes	Yes
Clustering	Electoral District	Electoral District	Electoral District	Electoral District
Time Fixed Effects	Yes	Yes	Yes	Yes
Country-of-Origin Fixed Effects	Yes	Yes	Yes	Yes
Arrival-year Fixed Effects	Yes	Yes	Yes	Yes
Unobserved HH Heterogeneity	Yes	Yes	Yes	Yes
Neighborhood Fixed Effects	Parish	Parish	Parish	Parish

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect) for two subsamples based on gender of the household head. In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. In specifications (iii) and (iv), we control for unobserved household heterogeneity using a random effects estimator. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have business or economics education and at least some college attendance. We consider a balanced sample of 4,061 refugee immigrants. Medium-term effects refer to financial behavior in the period 1999-2003, while longer-term effects refer to 2003-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table VI: Long Shadow Effects of Having Neighbors with Quantitative Education and College Attendance: Full Observation Period, Medium-Term, and Longer-Term

	Saving for Retirement		Stockholding		Saving for Retirement		Stockholding	
	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
<i>Panel A: Full Observation Period</i>								
Fin Lit Share	0.27525* (0.1520)	0.26714* (0.1572)	0.31259* (0.1601)	0.31522* (0.1664)	36513	36513	36513	36513
Observations								
<i>Panel B: Medium-Term</i>								
Fin Lit Share	0.34296** (0.1635)	0.09722 (0.1652)	0.37303** (0.1693)	0.13902 (0.1737)	20303	20303	20303	20303
Observations								
<i>Panel C: Longer-Term</i>								
Fin Lit Share	0.19884 (0.1696)	0.47743*** (0.1708)	0.24087 (0.1789)	0.53712*** (0.1759)	36513	36513	36513	36513
Observations								
<i>Household Controls</i>	Yes							
<i>Clustering</i>	<i>Electoral District</i>							
<i>Time Fixed Effects</i>	Yes							
<i>Country-of-Origin Fixed Effects</i>	Yes							
<i>Arrival-year Fixed Effects</i>	Yes							
<i>Unobserved HH Heterogeneity</i>	No	No	Yes	Yes	No	Yes	Yes	Yes
<i>Neighborhood Fixed Effects</i>	<i>Parish</i>							

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect) for various sample periods: the full sample (1999-2007), the medium term (1999-2003), and the longer term (2003-2007). In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. In specifications (iii) and (iv), we control for unobserved household heterogeneity using a random effects estimator. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who have quantitative education and at least some college attendance. We consider a balanced sample of 4,061 refugee immigrants. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table VII: Long Shadow Effects of Having Retirement Savers as Neighbors: Full Observation Period, Medium-Term, and Longer-Term

	Saving for Retirement	Stockholding	Saving for Retirement	Stockholding
<i>Panel A: Medium-Term</i>	(i)	(ii)	(iii)	(iv)
Fin Lit Share	0.11534 (0.0834)	0.02162 (0.0838)	0.14064 (0.0862)	0.04769 (0.0869)
<i>Observations</i>	20303	20303	20303	20303
<i>Panel B: Longer-Term</i>	(i)	(ii)	(iii)	(iv)
Fin Lit Share	0.17644* (0.0934)	0.17982** (0.0872)	0.20113** (0.0957)	0.23120** (0.0902)
<i>Observations</i>	16210	16210	16210	16210
<i>Household Controls</i>	Yes	Yes	Yes	Yes
<i>Clustering</i>	<i>Electoral District</i>	<i>Electoral District</i>	<i>Electoral District</i>	<i>Electoral District</i>
<i>Time Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Country-of-Origin Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Arrival-year Fixed Effects</i>	Yes	Yes	Yes	Yes
<i>Unobserved HH Heterogeneity</i>	No	No	Yes	Yes
<i>Neighborhood Fixed Effects</i>	<i>Parish</i>	<i>Parish</i>	<i>Parish</i>	<i>Parish</i>

Note: This table presents coefficient estimates from linear probability models of participation in saving for retirement through private accounts, and in stockholding (direct or indirect). In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. In specifications (iii) and (iv), we control for unobserved household heterogeneity using a random effects estimator. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. The share of financially literate neighbors refers to the initial electoral district of placement and is defined as the share of natives, as well as immigrants residing in Sweden for at least 20 years, who save for retirement in a private retirement account. We consider a balanced sample of 4,061 refugee immigrants. Medium-term effects refer to financial behavior in the period 1999-2003, while longer-term effects refer to 2003-2007. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.

Table VIII: Long Shadow Effects of Having Neighbors with Economics/Business Education and College Attendance on Various Outcomes: Medium-Term and Longer-Term

	Working in the Financial Sector			Earnings			Unemployed			Mover
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)			
<i>Panel A: Medium-Term</i>										
Initial Fin Lit Ext	0.02578 (0.0348)	0.02469 (0.0338)	0.05743 (0.4455)	0.16048 (0.4884)	-0.27936 (0.2696)	-0.25451 (0.2740)	-0.17966 (0.2832)			
Observations	19342	19342	19342	19342	17671	17671	4061			
<i>Panel B: Longer-Term</i>										
Initial Fin Lit Ext	0.07164** (0.0350)	0.06962** (0.0346)	-0.01394 (0.4163)	-0.03795 (0.4535)	0.37155 (0.3254)	0.35299 (0.3269)	-			
Observations	15697	15697	15697	15697	14377	14377	-			
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes			
Clustering	Electoral District	Electoral District	Electoral District	Electoral District	Electoral District	Electoral District	Electoral District			
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes			
Country-of-Origin Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes			
Arrival-year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes			
Unobserved HH Heterogeneity	No	Yes	No	Yes	No	Yes	No			
Neighborhood Fixed Effects	Parish	Parish	Parish	Parish	Parish	Parish	Parish			

Note: This table presents estimates of the determinants of different labor market outcomes estimated using a linear probability model. In all regressions, we control for household characteristics, arrival-year fixed effects, country-of-origin fixed effects, and neighborhood fixed effects defined at the parish level. Standard errors are clustered at the electoral district level (1,428 cells) and reported in parentheses. Financial literacy externalities are defined in terms of the share of neighbors (natives and immigrants who have been in Sweden for at least 20 years) in the electoral district of initial placement who had a business/economics education and had attended college. In specifications (ii), (iv) and (vi), we control for unobserved heterogeneity using a random effects estimator. Earnings are defined as the sum of labor income, entrepreneurial income and taxable employment-related transfers. In specifications (i)-(vi), we condition on having positive earnings. The sample is a balanced sample of 4,061 refugee immigrants. Panel A presents the results for effects on outcomes over the medium-term (1999-2003), while Panel B reports results for the longer-term (2003-2007). Statistical significance at the 10, 5, and 1 percent levels are indicated by *, **, and ***, respectively. Source: Author computations using LINDA and STATIV data from Statistics Sweden.